

## REPLY TO DR. SCOTT

By J. G. PRATT

I would be happier if Scott's rejoinder had made it unnecessary for me to say anything further. Unfortunately, this is not the case. For maximum efficiency in dealing with the new issues Scott has raised I will put my remarks in paragraphs numbered to correspond to his.

1. I regret the necessity of pointing out an evasiveness and a lack of candor in Scott's statements. He speaks of a note that was added after the first draft of the paper was completed. I took this (as I think most readers would do) as referring to a footnote, and I looked in vain for it in the final manuscript that was sent to me for reply. Further searching revealed that the "note" is the first sentence of a regular paragraph of their paper (see p. 172). Taken as a whole, this paragraph gives much stronger emphasis to the hypothesis of unconscious manipulation than does the single-sentence "note" quoted by Scott. Furthermore, the final version of the paper sent to me shows that at a number of places words like "fraud" and "trick" were canceled by x's and such words as "card misplacement" were inserted instead. This indicated to me that the authors were backing away from imputing conscious fraud to Woodruff and were emphasizing instead a misplacement of cards on an unconscious basis. Is Scott now saying that these changes were insincerely made, perhaps motivated by a fear of incurring a libel action? And is he now attempting to change his position back to the one held in an earlier version of their paper through the equally insincere device of saying that my reply has knocked out the hypothesis of unconscious manipulation and thus I am left with only the hypothesis of conscious fraud, a conclusion that he will not dispute? Scott cannot have it both ways, for I did *not* reach the conclusion of fraud that he is willing to leave unchallenged. I stated near the beginning of my paper that the fraud hypothesis was totally unacceptable to me, and this is still my position. An accusation of experimenter fraud has no place in science unless it is supported by evidence amounting to indisputable proof. Science cannot survive if every research finding

can be nullified by merely suggesting deliberate fraud, a possibility that always exists.

2. I was not proposing that Scott should carry out the analyses I suggested to discriminate between ESP and card manipulation. Rather, I only pointed out that the critics had not done so. My purpose was only to bring out the telling fact that Medhurst and Scott had shown no concern regarding ways in which their own non-ESP interpretation of their finding might be subjected to statistical test. Scott's lack of apology for their not having done the relevant analyses (those I suggested or others yet to be designed) can only be taken as supporting my statement that the critics were committed in advance to the non-ESP interpretation and were therefore not interested in studying the data further.

3. Acting on my suggestion, Scott found that the correlation of run scoring rates in the E- and M-piles is significant, as it should be on the ESP hypothesis. I pointed out the need for caution in interpreting this result in view of the small correlation that might be generated by card manipulation, but it seems unlikely that this can explain the finding. I think Scott is too ready to give up in the face of difficulties he sees confronting the computer simulation test I suggested. Instead of excusing ourselves on the ground of not knowing the precise value of  $n$ , why should we not have the computer shift cards to duplicate precisely the distribution of run scores obtained in the experiment? We know now that the correlation in the test data is significantly positive, so we do not need to be concerned with a kind of shifting that would give a negative correlation. (It does not escape my notice that Scott is offering to pass on our data to anyone willing to attempt this analysis. I hereby accept his offer and ask that the records be returned to me.)

4. The two new tests that Medhurst and Scott carried out were not concerned with the ESP hypothesis. It is not surprising that I did not improve on their performance, since they had the data while I (as explained in the paper) did not have access to the records. I do not agree that Scott has shown that none of my proposed tests are useful in helping to discriminate between ESP and card manipulation, but say only that he has not recognized their relevance. Even so, I did not claim that my suggestions exhausted the possibilities.

By my standards it would be appropriate to keep the issue open and to expend a great deal of further thought and effort upon it before leveling an accusation of conscious fraud against a young scientist, one who has made a distinguished career in academic psychology during the third of a century that has passed since we carried out our experiment.

5 and 6. I had, in principle, nothing against the use of all the data (the full range of run scores) by Medhurst and Scott. I only meant to point out that they were not explicit regarding how they reached their decision to do so instead of following the example set by Hansel of using only the runs with scores of 6 and above. Scott has now demonstrated that their analysis is significant at nearly the same level both ways. This shows that my concern was unnecessary, but it does *not* show that it was unjustified.

7. Parapsychologists now agree that no single experiment can "establish" ESP, and Woodruff and I did not claim when we published our report that our findings established ESP. What we did claim is that the findings supported the ESP hypothesis, since we could offer no normal explanation of the results under the experimental conditions. This situation is not, in my view, changed by the Medhurst and Scott finding. They have presented a secondary result that would be expected as a chance occurrence once in 84 analyses of random data, and they are willing to impute fraud to Woodruff on this basis. I say that they are showing undue and unseemly haste in so doing. In my judgment it is far more justified to say that their minor statistical effect does not strain the chance hypothesis to the breaking point than it is to choose the alternative of conscious fraud that they recommend. In any other field of science their criticism would be dismissed as unworthy of a second thought. Why should the standard be different in parapsychology? Are the parapsychologists themselves partly to blame for paying attention to such non-scientific discussions of their findings?

8. I was referring only to Hansel's point of view as regards the Pratt-Woodruff experiment. I think the statement is correct when taken in that limited sense, as intended.

9. I admit to a personal difficulty regarding taking seriously the possibility of "visual and aural" cues to key-card placements from one run to another *under the conditions of the experiment*. My own

opinion is that these possibilities were the same for all the subjects, in the sense that they did not exist for any of them. The claims to the contrary that have run through the Hansel-Medhurst-Scott criticisms have been based upon pure and unsupported speculation. The so-called demonstrations of the feasibility of obtaining and using such cues in the STM procedure all lack validity due to the absence of evidence of duplicating exactly the essential experimental conditions. Hansel has described a demonstration (*ESP: A Scientific Evaluation*, Scribner's, pp. 97-100) in which he coached the "subject" regarding how to handle the key cards so that the "experimenter" could keep track of them and shift some of the cards on his side of the screen as he was putting them down or later during the recording. The case against Woodruff has been made throughout on a "What if . . . ?" basis. Anyone can knock down the results of any experiment if an objection resting upon supposition and speculation is sufficient.

10. Hansel had only one hypothesis: that ESP meant "error some place." Thus his groping was not among different hypotheses, but over the data to find some effect which he could construe as supporting a normal explanation consistent with the experimental conditions. It is noteworthy that the experimental set-up and procedure were accepted by him as adequate to rule out the usual range of experimenter errors as well as conscious fraud on the part of the subject or by me. Thus his "groping" was of necessity narrowed to searching for something that he could interpret as evidence that Woodruff cheated.

11. Woodruff and I were only concerned in 1961 with the case that Hansel made against our findings. We had no reason to anticipate at that time the findings that Medhurst and Scott subsequently brought out by their analysis. I did not say in my paper that the later findings made no difference, but only that the matter had not been studied sufficiently by Medhurst and Scott to justify their criticism. They were the only ones to whom the data were available and they must accept full responsibility for their statements. I object strongly to their all-too-eager willingness to impute conscious fraud to Woodruff, as Scott now says they really intended to do. I can rationally deny that their paper "moves the balance at least some

distance toward Hansel's hypothesis" inasmuch as Scott has already found the positive correlation I predicted on the ESP hypothesis, and further analyses remain to be done that may further show that card manipulation can not reasonably account for their effect. Further investigations may thus reveal that their finding actually moves the balance toward the ESP hypothesis, since the original evidence brought out in our report may in the end be strengthened by independent statistical evidence for ESP from this secondary effect.

12. I confess that I did not always understand why Woodruff wanted each additional bit of information that he requested from Medhurst and Scott, or from Scott after Medhurst's death. But I submit that Woodruff may have had reasons that were sufficient from his point of view and which he did not wish at the time to reveal to the critics. I suggest that the circumstances were such that we should allow Woodruff to be the sole judge of what information was appropriate to his needs. Without knowing what use Woodruff intended to make of the data he was seeking, how could Scott decide what was and what was not relevant to the reply under consideration?

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As originally written, these comments ended with the preceding paragraph. Then I was extended the editorial courtesy of an opportunity to read in manuscript (and later in galley proofs) the "Comments" of Dr. J. B. Rhine on "Security Versus Deception in Parapsychology," the article that subsequently appeared in the *Journal* (March, 1974).

Since I was invited to offer my comments on that article, I made two suggestions. One was that it was inappropriate to publish those editorial comments *before* publishing a major controversy to which they were obviously relevant. The other comment was that it was not proper, if the article should be published first, to include references to the controversy which might prejudice the outcome of the further stage of criticism and reply awaiting publication. I said that if my suggestions were not accepted I reserved the right to comment upon Rhine's discussion of the Pratt-Woodruff research and the Hansel and Medhurst-Scott criticisms of it. It has unfortunately become necessary for me to exercise that privilege.

My comments will deal with two points: (a) a correction of fact

in Rhine's statements about the Pratt-Woodruff testing procedure; and (b) a fundamental difference of opinion regarding scientific evidence and the advancement of knowledge in parapsychology.

(a) Of the Pratt-Woodruff series, Rhine says on pp. 108-9 "... the keys were to be rearranged as randomly as possible by the subject before each run, under the continual observation and collaboration of E-1. . . . The mere fact that the actual conduct of the experiment was such that trickery was a conceivable possibility qualifies it for discussion here [as an example of experimenter deception, even though not proved]." As Woodruff and I were careful to point out in our reply to Hansel in 1960, there was no effort to achieve random permutations of the key cards from run to run, and we were careful not to claim in the report that the key cards were *randomized*. The objective was simply to take the cards from the pegs and replace them in an order that was not known to Woodruff, and this aim was accomplished by having the subject replace the cards in a new order without giving Woodruff any indication of the new arrangement. In the final subseries of 400 runs, I mixed the key cards and returned them face-inward to the pegs. The results were at the same level of success as in the earlier subseries. The point is that Woodruff and I avoided claiming that randomness of five cards can be achieved by hand mixing. A different order of the key cards was all that was required to keep Woodruff in the dark regarding their positions. The manner of replacing the key cards was a weakness in the experimental procedure only if one accepts the Hansel and Medhurst-Scott findings as evidence that Woodruff kept track of some of the key cards and used this knowledge to score "hits" by misplacing a target card occasionally. Since Rhine says he does not accept the fraud hypothesis, why does he say that the method of placing the key cards was inadequate for its purpose? The clue to the answer is to be found in what follows next.

(b) The fundamental difference of opinion is one concerning the nature of science and the place of the controlled experiment in the advancement of scientific knowledge in parapsychology. On the one hand there are those who set as a goal of research achieving a fraud-proof experiment. In parapsychology, a confirmatory ESP experiment (they say) should, to meet the highest scientific requirements,

exclude every conceivable alternative hypothesis. Unless all possibilities of error and even conscious fraud on the part of the experimenter are ruled out it cannot be concluded that significant results establish ESP. In its most extreme form this view holds that psi investigations must rule out experimenter deception as even a conceivable possibility. It now appears, after the publication of Dr. Rhine's "Comments" in the last issue of the *Journal*, that this position can be identified as the Price-Hansel-Medhurst-Scott-Rhine conception of parapsychological research.

On the other hand, there is the point of view which says that a fraud-proof experiment in parapsychology is not attainable, since a determined critic can always conceive of a normal explanation. All he needs to do is to ask a meaningful question of the type: "What if . . . ?" The blank space can always be filled in by words pointing to some conceivable normal action which could have produced the results and which was not excluded by the process of collecting, analyzing, and reporting the data. Henry Sidgwick recognized, in his first Presidential Address to the Society for Psychical Research, that the research workers could eventually make their safeguards so strong that the critics would be compelled to accuse the investigators of fraud, but never strong enough to prevent their doing so. Robert Thouless has been the most effective spokesman for this point of view about parapsychology at its present stage of development. In identifying it as my own position, I am only echoing Thouless's statements on the issue. From this point of view, the test of a good experiment is not whether it escapes criticism for a long time, such as the three decades since the Rhine precognition research that he cited. (It might have escaped criticism merely because it went unnoticed by those who might have criticized it if they had felt challenged to do so.) Rather, an experiment is valuable to the degree that it suggests further lines of research and proves in time to have been a reliable indicator of directions in which further advancements of scientific knowledge would be made. For parapsychology, the important question is not whether the research workers have arrived but whether they are on their way.

It is a side issue to demand at this stage an answer to the question whether the Pratt-Woodruff experiment did or did not establish

ESP. This question is one that is framed in terms of the misconception, discussed under (b) above, of the scientific process as it pertains to parapsychology. It is much more to the point to ask whether that research properly belongs in the mainstream of experimental efforts in the field, efforts that have advanced us thus far toward a growing knowledge and understanding of an aspect of reality not heretofore encompassed by normal science. To that question the answer is yes.

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